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#### IV.—RHYTHMIC ACCENT IN ANCIENT VERSE. A REPLY.

This paper is intended as a reply to the criticisms made upon my theory of ictus by Professor Hendrickson in this Journal, vol. XX, No. 2, pp. 198–210. With great acuteness and learning, this scholar advances numerous objections to my own view of ictus, besides adducing considerations which he thinks favor the traditional view. To none of these can I concede any validity.

Before taking up the points of Professor Hendrickson's paper *seriatim*, I shall permit myself in the briefest possible way to recapitulate the arguments of my original article as published in this Journal, vol. XIX, p. 361 ff. My object was to show that the evidence points strongly to the conclusion that ictus was *not* stress, and that it *was* merely quantitative prominence. My line of reasoning was as follows:

1. Latin was a quantitative language, i. e. the distinguishing characteristic of Latin words was found in the quantitative aspects (time-aspects) of the syllables composing them, whereas in English the distinguishing characteristic of a word is found in the stress upon one or more of the syllables (i. e. accents, primary or secondary). Latin, as an eminent phonetician who has given his adhesion to my theory puts it, was a language of "level stress." In Latin, therefore, since the stress was so slight, the quantitative aspects of syllables loomed large. In English, on the other hand, since the stress is so heavy, the quantitative aspects of syllables are hardly felt.

2. As a result of the quantitative character of the Latin language, I pointed out how natural it was that quantity should be made the basis of the rhythm of Latin verse,—just as natural as that in English stress-accent is made the basis of poetry.

3. Assuming that Latin poetry is quantitative, i. e. that a line of Latin poetry consists in a rhythmical arrangement of long and short syllables, I then proceeded to argue that, if Latin poetry was so felt, i. e. if the psychological element which appealed to the attention and characterized the rhythm was a time-element, then there could not simultaneously exist another psychological

element characterizing the rhythm. In other words, I contended that

Barbarico postes auro spoliisque superbi

could not in the Roman consciousness or any other be felt simultaneously as

— ∪ ∪ | — — | — — | — ∪ ∪ | — ∪ ∪ | — —

and as

' × × | ' × | ' × | ' × × | ' × × | ' ×.

I took this ground not only because it seemed to me theoretically obvious psychologically, but because it seemed to me abundantly confirmed by experience, and because the existence of a second principle of rhythmical grouping (i. e. stress as well as time), being superfluous, seemed impossible. To my mind, it made no difference whether this supposed stress were strong or faint. If it be perceptible enough to be apprehended in consciousness as the basis of rhythmical grouping, then I meant to say that any other basis, e. g. quantity, could not exist simultaneously. What I meant to do was to present a logical διλημμα, viz.: Is Latin verse metrical or accentual? It can not be both, was my conviction.

4. I also urged, as a weighty argument against the traditional stress theory of ictus, the fact that it involved the importation of a stupendous artificiality into the reading of Latin poetry. The poet's art, I maintained, never presupposes on the part of his readers the possession of a key to unlock the secret trick of his versification. The poet simply takes the choicer elements of familiar speech, uses them in their ordinary value and equivalence, and so arranges them that the rhythmical scheme to which they are intended to conform is immediately obvious to any one who can correctly pronounce the words as ordinarily uttered. At least, this is true of all languages of which I have ever heard, and poetry of any other nature is to me unthinkable. Here again it is of no avail to urge that the artificial stress thus introduced was faint. If it was apprehended in consciousness as the basis of the rhythmical structure of the verse, it must have involved a conscious effort to produce it. Whether this effort was small or great is immaterial.

5. Lastly, I endeavored to show that there nowhere exists, either in the extensive discussions of the Latin grammarians and metricians or elsewhere, any evidence that ictus was stress.

Such was my reasoning. Professor Hendrickson (p. 212) calls my view an "arbitrary assumption." Assumption it certainly

was. That an irrefutable demonstration of its truth was established, I do not maintain; but that it was arbitrary, in any sense of the word known to me, I can not concede. The facts and reasoning of the first four of the above five points still remain unchallenged, and seem to me still so weighty as to call for decided refutation before one proceeds to build an argument concerned solely with my fifth point, as Professor Hendrickson does. Granting all that he urges in that connection (and I can not grant a single point),—granting that we find facts which seem to favor the conception of ictus as stress,—we are still in the midst of irreconcilable contradictions. These contradictions seem to me so obvious and so serious as to suggest that we really do not advance appreciably toward the solution of the problem before us, unless we either adopt some theory of ictus consistent with my first four propositions above enumerated, or else eliminate the propositions themselves. Professor Hendrickson has done neither. My four fundamental theses pass unchallenged, while of the points raised against my view of ictus not one is sound. Some of them are not inconsistent with a stress theory, but not one necessarily makes for such a view.

I proceed to the discussion of Professor Hendrickson's arguments in detail. His first is drawn from Aristoxenus's *Elementa*, §§16, 17, 31. I quote the Greek: §16, *ὃ σημαίνόμεθα τὸν ῥυθμὸν καὶ γνῶριμον ποιούμεν τῇ αἰσθήσει, πούς ἐστιν*, 'That by which we indicate the character of the rhythm and make it intelligible to the perception is the foot'; §17, *τῶν δὲ ποδῶν οἱ μὲν ἐξ δύο χρόνων σύγκεινται, τοῦ τε ἄνω καὶ τοῦ κάτω, κτλ.*, 'Some feet consist of two times, the up and the down' [i. e. they are marked by an up and a down beat]; §31, *τῶν δὲ ποδῶν ἐλάχιστοι μὲν εἰσιν οἱ ἐν τῷ τρισήμῳ μεγέθει*. 'τὸ γὰρ δίσσημον μέγεθος παντελῶς ἂν ἔχοι τὴν ποδικὴν σημασίαν', 'The smallest feet have a magnitude of three morae; for feet of two morae would involve too frequent recurrence of the distinguishing characteristic (*σημασία*) of the foot.' Here Aristoxenus distinctly declares that feet of *two morae do not exist*<sup>1</sup> in Greek, and alleges a reason—viz. that, if the pyrrhic were to be used, its employment would involve too frequent recurrence of the distinguishing characteristic of the foot. This distinguishing characteristic, urges Professor Hendrickson, could be nothing but stress. But I can not admit

<sup>1</sup>How Professor Hendrickson can say (p. 201, n.) that Aristoxenus "assumes" the pyrrhic foot, when Aristoxenus flatly denies its existence, is not clear to me.

the legitimacy of any argument based upon the assumed character of anything which we are specifically assured does not exist. The character which a non-existent entity would have, if it existed, seems to me too speculative a problem to prove of value in our discussion. Then, what if the reason alleged by Aristoxenus for the non-existence of the pyrrhic as a fundamental should not be the correct one? When the great metrician is dealing with *facts* of music and rhythm, I am ready to attach the greatest importance to his utterances; but when he indulges in speculation as to the grounds for the non-existence of an imaginary rhythmical form, I consider that his theories are to be taken for what they may be worth—nothing more. Or again, granting the correctness of Aristoxenus's explanation, might we not easily believe that, in an imaginary pyrrhic sequence like  $\delta \delta' \text{ ἴδε μάλ᾽ ἀπολύ περ}$ , the first syllable of each foot would be felt as the distinguishing characteristic (the *σημασία*) of that foot? That the initial syllable of a group is psychologically prominent needs no demonstration, and is shown abundantly by the persistence in the Romance languages of unaccented initial syllables of Latin words where the rest of the word has gone to wreck. Still, I am disinclined to accept this explanation. To me it seems much more likely that the reason assigned by Aristoxenus for the non-existence of the pyrrhic as a fundamental foot is a wrong one. To my mind, it is much more likely that the reason is the same as that for the non-existence of the tribrach and the proceleusmatic—viz. lack of quantitative variation of the foot-elements, which precludes the existence of any *σημασία* or distinguishing characteristic. I can not, therefore, admit the justice of Professor Hendrickson's remark (p. 200), that my "identification of thesis [ictus] with quantitative prominence is susceptible of theoretical refutation out of the words of the great master of ancient rhythmical theory."

Professor Hendrickson's second argument (p. 200) is based upon the doctrines set forth by Aristoxenus in §4 of the *Elementa*, to the effect that a given syllable-group (e. g.  $\cup\cup\cup$ ,  $—$ ,  $—\cup\cup$ ,  $\cup\cup—$ ) may be distributed into different combinations according to the nature of the rhythm in which it occurs. This doctrine is sufficiently familiar, and needs neither exposition nor defence. But Professor Hendrickson's interpretation of its significance, I can not accept. He cites Pindar, *Pyth.* 2, 1 *Μεγαλοπόλεις* 3, and urges that here "we have to do with a difference of rhythmic effect which nothing but a rhythmic accent or intensity [stress]

could bring out." I fail to see this. This ode was, of course, written for musical performance, and the time was, presumably, made clear to the performers by the playing of a few measures before they began to sing the text. This would at once reveal the trochaic rhythm, and the two initial tribrachs would at once adapt themselves in consciousness, as Aristoxenus explains, to the trochaic character, entirely without the help of an artificial stress. Or let us assume that the words were read. In that case it would certainly be necessary to read the line partly through, before one apprehended its metrical character, i. e. trochaic. The third and fourth feet, however, make this perfectly clear. Even Professor Hendrickson's theory that the first and fourth syllables of *Μεγαλοπόλις* were stressed, compels us to assume that the reader first reads the line through before locating his stress. Until he has done this he can not determine whether he is to stress *Μέγαλοπόλις* or *Μεγάλοπολις*. In rhythmical schemes so complex as those of Pindar and intended, primarily, not for reading, but for singing, it is obvious that, without the help of the musical accompaniment, the metrical structure will not be immediately apparent from the beginning, unless the line opens with fundamental feet. In the example cited by Professor Hendrickson the question is whether, after the reader has advanced far enough in the line to see its trochaic character, and then goes back, he must stress the initial syllable of the two tribrachs in order to feel their trochaic character or can feel it without. Knowing the rhythm, it seems to me that the reader can feel these two tribrachs as trochees (i. e. prominent at the beginning, not the end of the foot), just as easily as though the line began with two trochees and then two tribrachs succeeded. In the latter case the quantitative prominence inherent in the long syllable of every trochee (its *σημασία* or distinguishing characteristic) is, I believe, psychologically transferred to the initial part of the tribrach, just as in English in iambic rhythms we may have  $\times \times$  or even  $'\times$  in place of the iambus ( $\times'$ ), in which case the dominant iambic character of the line makes us psychologically transfer the feeling of accentual prominence to the unaccented syllable. Instances are so frequent that I may dispense with citation.

I turn to Professor Hendrickson's next argument, which is based upon psycho-physical experiments upon the rôle played by stress. Thus, he urges that "it has been shown that a uniform series of unaccented sound-impressions of variable duration ( $—\sim$

or — ∪ ∪) tend to combine with quantitative prominence greater degrees of intensity." But to such experiments I can not attach the slightest importance as bearing upon the subject under discussion. The experiments referred to were made upon Teutonic subjects, where lack of sensitiveness to quantitative differences is inherent in the strongly stressed character of the German and English languages. Their rhythmic sense is a sense for intensities, not for quantity. It would have been passing strange if, in the irresistible tendency toward rhythmic grouping suggested by the recurrence of the same uniform series, the minds of the subjects referred to should have failed to associate a sense of intensity with the quantitatively longer elements of these series. But one may well ask whether, if similar experiments could be made upon a Roman, precisely the reverse associational process would not manifest itself.

Professor Hendrickson adds (p. 202) that, in accepting this principle in verse (viz. the principle of artificial stress-accents), it need not be thought that we introduce an element of violent stress that shall run athwart the natural word-accents. But, as I have already indicated above (p. 413), the question of strong or weak stress has nothing to do with the question at issue. If the stresses are apprehended in consciousness as the basis of the rhythm, we then get an accentual poetry, and why such a poetry should have been constructed on the severe quantitative principles of classical verse would be an inexplicable mystery.

Professor Hendrickson next advances an argument drawn from the recognition of the dipodic arrangement of many of the ancient classic metres. "There is but one principle," he declares, "by which such a grouping can take place, and that is by intensity on the one or the other of the elements of the group." But the ancient metricians who recognize the dipodic grouping for the Latin senarius, for example, are explicit in attaching other significance to the dipodic grouping. Thus Terentianus Maurus, de Metris, 2249, connects it with the fact that in the 2d, 4th, and 6th feet (except in free iambic metres) a pure iambus is necessary, an explanation not only possible, but very rational. Denying, therefore, as I must, Professor Hendrickson's argument that the very existence of dipodies necessitates the assumption of stress on alternate feet, I can find no support for his interpretation (p. 203) of *percussio* (used by Quintilian in IX 4, 75) as meaning 'stress.'

Nor can I concede to Professor Hendrickson's next argument any validity. He urges (p. 204) that, in denying to such terms

as *pedem supplodere*, *plausus* or *pulsus pedis*, *strepitus digitorum*, *pollicis sonus*, any significance as denoting vocal stress, I overlook the intimate association of mind and muscular expression. "That these terms," he adds, "indicative of muscular contraction, corresponding to the prominent part of the foot, afford indubitable evidence of the presence in the mind of recurrent pulsations of intensity [stress] will scarcely be denied. But what the mind feels the muscular organism reproduces. It is therefore a matter of indifference from an abstract point of view what muscles are involved. . . . the performer finds the outlet for the recurrent sensations of intensity in the muscular response of the whole vocal organism." Accordingly, his conclusion is, that the designations above cited do possess significance as indicating vocal stress. But while I can admit that these terms would be perfectly natural designations to apply to methods of denoting such recurrent pulsations, I fail to see that they necessarily can apply only to recurrent pulsations of stress and can not just as naturally apply to recurrent pulsations of quantity. Hence I am still constrained to deny to the employment of these terms any evidence in support of the stress theory of ictus. Were we to grant Professor Hendrickson's premises and conclusions on this point, however, we should in effect simply have from his hand another proof that such a thing as quantitative poetry can not exist.

Professor Hendrickson next turns to the evidence to be found in the verse of Plautus and Terence. "But it will not require elaborate proof," he says, "to maintain that in the verse of Plautus and Terence there is a rhythmical accent of essentially the same nature as the word-accent." Klotz, *Grundzüge*, p. 88, is cited in support of this position. Thus, in a few brief words, without discussion, is settled one of the great controversial problems of Plautine and Terentian metric.<sup>1</sup> The phenomena of the iambic law, in their general manifestations at least, are sufficiently familiar. But that they are adequately explained, or that an actual consensus of opinion exists as to their cause, can hardly be maintained. Havet distinctly rejects the theory now widely current, that the phenomena of shortening are the result of verse-accent; while Crusius, *Literarisches Centralblatt*, 1891,

<sup>1</sup> Ritschl's view in his last years favored an obscure pronunciation of these shortened syllables; Corssen attributed the phenomenon exclusively to the 'Hochton'; Havet still holds to the theory of *breves breviautes*; others advocate the recognition of the influence of stress-ictus.



Sp. 212, says of Klotz's theories: "Mit dem metrischen Kürzungsgesetz freilich scheint in vielen Fällen der Knoten mehr durchhauen als gelöst." The force of this general criticism is well illustrated in Klotz's explanation of the shortening of the final long vowel of cretic words (e. g. *māchīnās*) when such vowel appears as the second short of the arsis (*Senkung*) in anapaestic rhythms, e. g. *mā|chīnās mō|lītus* (Persa, 385). On p. 66 of the *Grundzüge*, Klotz offers the following theory of shortening in words of this type: We may assume that at the beginning of an anapaestic line, e. g. ◡ ◡ ◡ ◡ ◡ ◡ ◡ ◡, there was a special stress to mark the beginning of a new verse (i. e. the first anapaest was stressed thus: ◡ ◡ ◡). This is a prodigious assumption by itself, but Klotz goes on to a more astounding one. He assumes that this initial stress on the first anapaest communicates itself by analogy to the following feet, so that when we have an example like *mā|chīnās mō|lītus* (above cited) in the fourth foot, there really existed a stress on *chī*, and that this stress accounts for the shortening of the -ās. Whether this is one of the theories of stress-ictus which Professor Hendrickson considers "proved to suffocation," I do not know. I only cite it as illustrating the desperate shifts to which one of our leaders in metrical theory finds it necessary to resort in order to reconcile the facts with his theory of iambic shortening. It illustrates also the wide divergence of opinion among our theorists. For while in the foregoing typical example Havet denies the influence of verse-accent in the phenomena under discussion, and while Klotz explains the shortening by the assumption of an *antecedent* supposititious stress as above detailed, other metricians (e. g. Brix, Skutsch) find the effective cause in a *following* stress-ictus.

The iambic law, as it has been current for the last thirty-five years,<sup>1</sup> is essentially as follows: "Any iambic syllable-sequence may become pyrrhic when the word-accent or verse-accent (stress-ictus) rests upon the syllable immediately preceding or immediately following the long syllable." It is of course plain that, if it is necessary to assume the existence of a stress-ictus in order to account for certain phenomena in Plautine metric, then ictus was stress. But can we fairly take the ground that no other explanation will suffice? The phenomenon in question occurs most frequently in iambic and cretic word-forms. In the iambic

<sup>1</sup> So far as I can discover, the first formulation of it as now current is in Brix, *Trinummus*<sup>1</sup> (1864).

word-forms there is no necessity of resorting to the theory of a stress-ictus to explain the shortening, since the word-accent alone is sufficient for this. Similarly, in cretic words may we not explain the shortening by the initial word-accent aided by the assimilative effect of the short syllable? That is, is it not phonetically rational to explain *temperi*,<sup>1</sup> when used as a dactyl, as the result of word-accent? That it is, seems evident to me, and is supported also by the fact that the same theory will also explain *mā|chmās mō|* in the Persa passage above cited, as well as other passages of which that is but a type. In those cases where the shortening occurs in iambic combinations within a word or in iambic combinations formed of two or more words, I should like to suggest that the phenomena of shortening may be satisfactorily explained by sentence-accent, phrase-accent, or secondary word-accent. An example of sentence-accent is perhaps to be seen in such expressions as *Sed quis hæc*; the influence of secondary word-accent may be seen in *et in dêterrêndo, tibi intêrpellatio, sorôr si ôffirmâbit*. In most of the cases cited by Brix and Klotz we can find an explanation of the shortening without resorting to a theory of an artificial metrical stress. This is true of every one of the scores of examples given by Brix in his *Trinummus*, *Einleitung*, p. 17. These can all be satisfactorily explained on the basis of recognized effects of Plautine word-accent and phrase-accent (one example: *voluptās mea*), without resorting to any theory of stress-ictus. That the ictus either precedes or follows the shortened syllable is, I believe, not a *causa efficiens*, but an *accidens*. That it is an *accidens* would seem plausible, in view of the fact that in most kinds of Plautine verse the very structure of the verse (viz. the large agreement between word-accent and 'ictus') requires that the shortened syllable come either immediately before or immediately after the so-called ictus (cf. Christ, *Metrik*<sup>2</sup>, §28 N., p. 21). Under these circumstances, to elevate into an impelling cause what may be only an interesting coincidence seems to me unjustifiable. At present my own feeling is that we can not go further than to formulate the law that unaccented final syllables in words and sequences of iambic and cretic form had a tendency to become shortened in certain feet. As a partial explanation of the phenomenon I would offer the reasons advanced so many times: the tendency for final syllables in Latin to become obscured, and the assimilating influ-

<sup>1</sup> Cf. *lūdicerē*, etc., in Ennius.

ence of the preceding short vowel, coupled with the initial word-accent. Could these causes have sufficed to make such syllables *anceps*, so that they were used as long or short at the option of the poet?

At all events, it can not be too strongly insisted upon that the phenomena of shortening are to be explained primarily as a reflection of the normal colloquial speech of Plautus's and Terence's day, rather than as a result of artificial metrical laws that tended to distort the living language. The syllables were used as short because they *were* short, not because the exigencies of the verse required the shortening of what was long in the spoken idiom. Cf. on this point Greenough (Harvard Studies, V, p. 57, and especially p. 71), who properly insists that the very nature of Plautus's audiences forbade any deviation from the recognized standards of familiar utterance.

Professor Hendrickson's next argument is based upon an interesting passage of Gellius, VI 7, "in which," he says, "inference concerning the accent of words is made from the rhythmical prominence which the syllables receive in the verse of the early poets." He adds: "if the reader will refer to the passage he will not be able to doubt that rhythmical accent is here invoked to determine word-accent." I can not believe, however, that this conclusion of Professor Hendrickson's will commend itself to others. In my judgment, no inference could be less justified. Evidently, if the rhythmical accent could be invoked to determine word-accent, Annianus would hardly have restricted himself to the consideration of a small handful of prepositional compounds. A consistent application of the theory would have included hundreds of words. Not only that, it would have proved the accentuation *exadvēsum* as easily as *exádvēsum*. Nor is it suggested in the Gellius extract that the pronunciations alluded to are *proved* by the metre of the Cistellaria passage and that from Terence's Phormio. These passages are cited merely as illustrating the application of the theory. That Annianus's principle was arbitrary and fantastic is shown by the fact that he claimed for it general applicability to compounds of *ad* in which *ad* has intensive force: *adpōtus*, *adprimus*, *adūro*, as well as *adfabre*, *adprobe*, *admodum*. It therefore seems to me entirely reasonable to interpret the coincidence of the ictus and the alleged accent in *exádvēsum* as something purely fortuitous.

Professor Hendrickson next passes to an argument based on the occurrence of lines in Ennius consisting of five spondees and

one dactyl. A conspicuous illustration of an abundance of spondee is found in Ennius (*Annales*, vv. 196-203, ed. Müller). Here in a continuous passage of eight lines we have four lines which contain each but a single dactyl. Professor Hendrickson asks: "Now, what could have made out of that cumbersome mass of syllables a literary form that should have been tolerable? Rhythm of recurrent stress,—and nothing but such rhythm." Professor Hendrickson admits that theoretically "a Latin verse consisted of an orderly and harmonious arrangement of long and short syllables," as I have myself urged; but in fact, he frankly declares (p. 206), the verses of Ennius and his contemporaries must have been accentual. They must have been characterized by "rhythm of recurrent stress and nothing but such rhythm." I freely admit that, if they were characterized by rhythm of recurrent stress, they could have been characterized by nothing else, and would observe again that, if we concede Professor Hendrickson's conclusions here, we simply admit that Latin poetry was accentual, not quantitative. But let us see what conclusions we are really justified in building on the eight-line fragment of Ennius above referred to. The passage is one which Professor Hendrickson came upon as he opened his Ennius at random. One naturally assumes that it may be taken as fairly representative of Ennius's technique. A cursory examination of Ennius's verse would easily have assured Professor Hendrickson that such is far from being the case. The passage in question is unique in its abundance of lines containing but a single dactyl. If we take the 412 perfect lines of the fragments of the *Annals*, as given in Müller's edition (including these eight lines under discussion), we find that on an average only about one line in seven has but a single dactyl, while in Virgil the average of such verses is one in ten. Moreover, Ennius's verse is lightened by the free use of lines containing five dactyls, whereas Virgil indulges but seldom in lines of this type. On the whole, the abundance of dactyls in Ennius's verse is, I believe, practically the same as in Virgil's. In support of this I offer the following data.

An examination of 412 lines of Ennius's *Annals* which are complete and perfect shows:

Lines containing but one dactyl,	65
“ “ two dactyls,	139
“ “ three dactyls,	129

Lines containing four dactyls,	57
“ “ five dactyls,	19
“ “ no dactyls,	3

An examination of 100 lines<sup>1</sup> of Virgil shows:

Lines containing but one dactyl,	10
“ “ two dactyls,	39
“ “ three dactyls,	35
“ “ four dactyls,	16
“ “ five dactyls,	0
“ “ no dactyls,	0

On an average, therefore, Ennius's verse shows 2.53 dactyls to the line; Virgil's, 2.57.<sup>2</sup> Any one who will read aloud any of the other longer fragments of the *Annals* can hardly fail to convince himself of their general conformity to the established canons of classical metric. Even in the passage before us, almost grotesquely abnormal as it is, it should be noted that, though we have four heavy lines, the other lines are much lighter than the average, two of them having three dactyls and one four. If, in addition, we assume, what is almost certain, that the lines preceding the beginning of this fragment and following its end were also light, we can hardly join Professor Hendrickson in his conclusion that "rhythm of recurrent stress and nothing but such rhythm" could have made even out of these eight lines a tolerable literary form. Much less can we join him in his implied conclusion that the same is true of the rest of Ennius's hexameters. For if true of them, it must be true of Virgil, unless one were to find in the difference between an average of 2.53 and 2.57 dactyls per line a justification for an altered attitude.

Passing on to the argument drawn from Quintilian's statement in I 10, 25, I can see in it nothing confirmatory of the stress theory. Quintilian observes: *atqui in orando quoque intentio vocis, remissio, flexus, pertinet ad movendos audientium adfectus*. To my mind, *intentio* and *remissio vocis* here naturally refer to words and phrases, not to syllables, and designate the increase and decrease of volume of sound ('rising' and 'falling' of the

<sup>1</sup> These lines were taken absolutely at random. Each was selected before examining its structure. No line was discarded. A fuller examination of Virgil's verse would doubtless give a slightly different average.

<sup>2</sup> For Cicero's 740 hexameters the average is 2.47; for Lucretius, vv. 100-115 (116) in Books I-VI show 2.66.

voice); at least, I am not prepared to interpret them as signifying stress and absence of stress until some definite reason is advanced in favor of this interpretation.

Similarly, I am entirely at a loss to see any warrant for interpreting the words of Marius Victorinus (Keil, VI 40, 14) as Professor Hendrickson does. The words are: *thesis positio pedis cum sono*, and the idea of stress is attached to the vague word *sono* in accordance with the psychological argument previously advanced by Professor Hendrickson (p. 201) and which I have already discussed (above, p. 416 f.). What Professor Hendrickson advances with regard to the significance of Marius Victorinus's definition of *thesis* (using the word in the opposite sense) as *depositio et quaedam contractio syllabarum*, also seems to me open to criticism. Professor Hendrickson thinks the words *contractio syllabarum* a recognition of the principle of metrical shortening under the influence of adjacent stress. But I question whether *contractio* is ever used for *correptio*; moreover, the definition is obviously intended to apply to all feet; and as it is manifestly impossible to maintain that the single short of the trochee and iambus, or the two shorts of the dactyl and anapaest are the result of any such shortening, it should be clear, I think, that no support for the stress theory of ictus can be based upon these words.

Only one other criticism of Professor Hendrickson upon my theory of ictus remains to be considered. Here Professor Hendrickson frankly confesses his own doubts as to the significance of what he adduces. I agree with him entirely in this feeling, and shall only undertake to confirm his opinion of the weakness of his argument. The point is this: In my original article I stated that no evidence existed to show that the tribrach and dactyl were stressed upon their second syllable in iambic verse (p. 380), and added (p. 381): "To my mind one of the strongest arguments against the stress theory of ictus is that the ancient metricians never alluded to the location of the ictus in resolved feet. If ictus was stress and the second syllable of the iambus was stressed in verse, then the location of this stress in resolved feet would have been one of the first questions to suggest itself to the metricians. Its consideration would have been inevitable. Yet they never once allude to it, though they enumerate frequently the various possible resolutions of the iambus." It is this missing evidence that Professor Hendrickson

undertakes, though hesitatingly, to supply. If a stressing of the resolved iambus as  $\cup\cup$  or  $-\cup$  can be established, it is obvious that my theory of ictus must fall. In support of such stressing, Professor Hendrickson cites Caesius Bassus (apud Rufinum, Keil, VI 555): ad Neronem de iambico sic dicit: 'Iambicus autem, cum pedes etiam dactylici generis adsumat, desinit iambicus videri, nisi percussione ita moderaveris, ut cum pedem supplodis, quam iambicum ferias; . . . quod dico exemplo faciam illustrius. est in Eunuchio Terentii statim in prima pagina hic versus trimetrus:

Exclusit, revocat: redeam? non si me obsecret.

hunc incipe ferire, videberis heroum habere inter manus.' This passage is given by Professor Hendrickson without comment, and its interpretation is left to the judgment of the reader. I shall here record mine. Bassus means to say that in iambic verse the dactyl is likely to fail of being felt as an iambus unless you so manage the beats, when you stamp the foot, as to strike the dactyl like an iambus,<sup>1</sup> i. e. Bassus is speaking of *artificial scanning* and means to say that in scanning it is necessary to bring down the foot on the second half of the dactyl, since that part must be the one prominent in consciousness. The difficulty, however, to which Caesius Bassus alludes, could hardly occur in continuous iambic rhythms,—only in isolated lines like the one cited, where at the outset, owing to the lack of definitive iambic ear-marks, the reader might cherish the illusion that he was dealing with the dactylic hexameter.

In a footnote to his discussion of the foregoing point, Professor Hendrickson observes (p. 210): "Clear evidence that the dactyl was thus scanned [ $-\cup$  in iambic verse] in the verse of Plautus and Terence may be derived from the plays themselves." Professor Hendrickson here touches again upon one of the great controversial questions of Plautine and Terentian metric, and confidently takes a position on that side of the question which is suited to the needs of his argument. I am well aware of the great names—Bentley, Hermann, Ritschl—that are linked with the theory in question; I am aware, too, of the intolerance which resents as sacrilege any disposition to question the naturalness or the charm of "jene harmonische Disharmonie" supposed to result

<sup>1</sup> I can not believe that Professor Hendrickson's constitution of the text of the passage is sound, and should myself follow Keil; still, this point is immaterial.

from the clash of word-accent and stress-ictus. But I must maintain that any such theory can hold only when it is first shown that ictus is stress. To assume that ictus is stress and then to turn for proof to the fact that the word-accent in Plautus and Terence largely agrees with the ictus, is the "vicious circle." Even that Plautus and Terence consciously aimed to secure agreement between the word-accent and ictus in any sense of that term, seems to some an exceedingly questionable proposition. That in some types of verse there is large agreement between word-accent and ictus (undefined) in certain parts of the line, is obvious; but to what extent it is intentional, and to what extent it is merely a coincidence, seems to many still an open question. Whatever decision be reached on this point, however, will not affect the integrity of my view as to ictus, unless it first be shown that ictus was stress. If the fact of conscious endeavor to secure agreement between ictus and word-accent be established, we can perfectly well content ourselves with recognizing that there was a tendency in certain types of verse to bring the word-accent into the quantitatively prominent part of the verse.

Professor Hendrickson's footnote on the Eunuchus passage has drawn me into a digression. I return to his point, viz. that there exists evidence in favor of the stressing of the resolved iambus thus:  $\cup\cup\cup$  or  $-\cup\cup$ . The evidence in favor of this proposition supposed to be offered by Caesius Bassus has already been considered. Professor Hendrickson also offers the following from Servius (Keil, IV 425, 8). Servius is treating of feet, and deals with a few elementary definitions. Among other things, he speaks of the distribution (as between arsis and thesis) of the syllables of three-syllable feet. This distribution, Servius says, must be determined by the accent (*hoc ex accentu colligimus*). If the accent is on the first syllable, the arsis<sup>1</sup> will have two syllables; if it is on the second syllable, the thesis<sup>1</sup> will have two syllables. Professor Hendrickson's comment on these words of Servius is this: "I need scarcely point out that *accentus* here refers to rhythmical accent [stress], not word-accent. For not once in the chapter does Servius confuse word and foot."<sup>2</sup> Nevertheless, I feel strongly the necessity of adhering to the

<sup>1</sup> Arsis is here, as often, used in the sense of the first part of the foot; thesis, of the second part.

<sup>2</sup> I can not see that the rest of the chapter affords any opportunity for such confusion.



interpretation of this passage which I advanced in my original article. In that (p. 368), I grouped together Servius, Pompeius and Julianus as uniting in a common conception of the foot as displayed by their definitions. I held that they all appeared to identify the foot with a word. This is specifically done by Pompeius and Julianus, who show their conception by their illustrations. I held that the above passage of Servius was to be interpreted in the same way. I still hold to that view. I do it because Servius, Pompeius, and Julianus evidently all drew *from a common source*<sup>1</sup>—Donatus—and may therefore, in a case like the present, fairly be presumed to represent a common tradition. Further support for my position is found in the fact that Sergius, who evidently also drew from the Donatus-well, exhibits the same confusion (Keil, G. L. IV 483, 14). But even apart from this, I should refuse to admit Professor Hendrickson's interpretation of the word *accentus* in the Servius passage as meaning stress. The word *accentus* is nowhere used in any sense except that of a permanent element of a word, the syllable made prominent in the pronunciation of a word whenever that word is uttered. I fail to see what right we could have to interpret the word otherwise than in its accepted sense.

I have endeavored to consider fairly the objections brought against my original paper by Professor Hendrickson. My object is not, primarily, to defend a position once taken. Before I read Professor Hendrickson's recent article, I felt entirely ready to be convinced that my attitude was wrong and could be proved wrong. Convincing proof of error would have been welcome, as it would have tended to establish a doctrine which has hitherto been practically nothing but an assumption, and which sadly needed some definite logical and historical basis. Yet I can not see that Professor Hendrickson has provided either.

Before concluding I must venture to advert to the closing portion of Professor Hendrickson's paper, in which he makes some remarks upon another publication of mine.<sup>2</sup> Professor Hendrickson expresses the belief that my conception of Latin

<sup>1</sup> This is shown by the internal evidence; see Keil, G. L. IV liii; and especially V 91; V 6; Teuffel-Schwabe, §409, 2; Fröhde, *Anfangsgründe der römischen Grammatiker*, Einleitung, p. 12.

<sup>2</sup> The Quantitative Reading of Latin Poetry, published simultaneously with my previous article in this Journal.

poetry could only lead us to look upon the metrical schemes of the poet from a lifeless, mechanical point of view. That I do not shrink from a reversion to such a mechanical conception, Professor Hendrickson thinks is shown by an utterance in the pamphlet referred to. I quote his next words: "There he [myself] affirms (with a dogmatism which the requirements of a school manual may excuse) that the ancients [Romans] felt the lesser Asclepiadean (as in *Mæcenas atavis*) thus: — — | — ∪ ∪ — | — ∪ ∪ — | ∪ —. Irrational spondee, choriambi, pyrrhichius—as though there could be any talk of feeling in such a hodge-podge of heterogeneous feet." But I had supposed that it would be obvious that by "ancients" I simply meant Servius and the other ancient metricians, who do assert that they felt the line precisely as I state. I am sure that Professor Hendrickson now acquits me of dogmatism on this point. The defence of the special views advocated in my little pamphlet must be left for another time and place.

ITHACA, Nov. 6, 1899.

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